Social Science Library
OCT 14 1952

Vol. 6, No. 3 September, 1952

Canadian Journal of Psychology

THE JOURNAL OF THE CANADIAN PSYCHOLOGICAL ASSOCIATION

EDITOR: JOHN A. LONG

ASSISTANT EDITOR: KATHLEEN M. HOBDAY

EDITORIAL ADVISORY BOARD

D. O. HEBB, *Chairman*

J. BLACKBURN, R. B. MALMO, N. W. MORTON, C. R. MYERS

CONTENTS

Psychological Research in Canada and in Canadian Defence: N. W. MORTON 103

Motivational and Emotional Characteristics of "Bright" and "Dull" Rats: WILLIAM R. THOMPSON and DALBIR BINDRA 115

The Dependence of Binocular Fusion on Timing of Peripheral Stimuli and on Central Process.
2. Asymmetrical Flicker: C. H. BAKER 123

The Psychologist in a Clinical Setting: R. WEIL 131

Irrational Fears in the Dog: RONALD MELZACK 141

Book Reviews 148

PUBLISHED QUARTERLY

MARCH - JUNE - SEPTEMBER - DECEMBER

THE UNIVERSITY OF TORONTO PRESS

$3.00 PER YEAR

ALL RIGHTS RESERVED

Canadian Psychological Association

1952-1953

COUNCIL

Honorary President, W. LINE, *Toronto*

EXECUTIVE: President, D. O. HEBB, Montreal; Past President, N. W. MORTON, Ottawa; President Elect, D. C. WILLIAMS, Toronto; Secretary-Treasurer, G. A. FERGUSON, Montreal.

DIRECTORS: R. H. SHEVENELL, Ottawa; LOUISE M. THOMPSON WELCH, Halifax; E. S. W. BELYEA, Vancouver; D. E. SMITH, Edmonton; R. B. MALMO, Montreal; B. M. SPRINGBETT, Winnipeg.

CONSULTING EDITORS: E. S. W. BELYEA (*British Columbia*); G. A. FERGUSON, CLAUDE MAILHIOT (*Quebec*); W. G. SCOTT (*Ontario*); W. H. D. VERNON (*Maritimes*); B. M. SPRINGBETT (*Prairie Provinces*).

Correspondence regarding subscriptions should be sent to the Canadian Psychological Association, 102 St. George St., Toronto 5, Canada.

Manuscripts or correspondence on editorial matters or advertising should be sent to the Editor, John A. Long, Ontario College of Education, 371 Bloor St. West, Toronto, Canada.

AUTHORIZED AS SECOND-CLASS MAIL, POST OFFICE DEPARTMENT, OTTAWA.

Vol. 6, No. 3 September, 1952

Canadian Journal of Psychology

PSYCHOLOGICAL RESEARCH IN CANADA AND IN CANADIAN DEFENCE[1]

N. W. MORTON
Defence Research Board, Ottawa

THERE IS a saying that you can discern an educated gentleman by being able to listen to him talk for an hour without learning his occupation in life. I must warn you—and perhaps my lapse is in some degree excusable in a professional gathering such as this—that I am quite frankly going to talk "shop" in a most ungentlemanly manner. In fact I am going to veer right to the extreme and to present a simple, vulgar example of the cobbler sticking to his last by talking about my own field of work and the things I believe I see from the vantage point of it. What I have to say may thereby not prove inspiring, and indeed may be something of a pot-pourri, but at least will have the merit of being fairly genuine.

The remarks which will serve as this presidential address I regard as constituting in part something in the way of an apologia. For the last five years I have been endeavouring to guide the work of certain organizations devoted to the development or prosecution of military and non-military psychological research. These organizations, which now employ in research a considerable number of psychologists and scientists in related fields, and which support psychological research in Canadian universities to a substantial extent, were created essentially as a result of the contribution made by you and your fellow members in the Second World War, when psychologists in Canada and elsewhere demonstrated under conditions of emergency to what extent their science could aid in solving the practical problems of making the most effective use of human resources in the armed services at that time. They constitute only a partial fruit of your labour then but yet I feel an important one, and because of this my own job has been in a sense to act as trustee of what I believe are assets of growing value in the professional sphere of psychology in Canada. This job has been, I can assure you, a most interesting one, because it has afforded contacts with psychologists across Canada, has involved experience in interdisciplinary relations, and has permitted working with defence forces and other kinds of government staffs who have proved to be refreshingly responsive and co-operative.

I am not sure where to begin, and perhaps it is not important, since I

[1]Presidential Address delivered at the Annual Meeting of the Canadian Psychological Association at Banff, Alberta, June 16–18, 1952.

shall undoubtedly ramble anyway. I should like to attempt some observations on the general situation of psychological research in Canada. I am not certain what its position was before the last war. I suspect that on the whole there was relatively little, and that it was concentrated mostly in two or three universities. It would be interesting to see a reliable well-documented pre- and post-war comparison in this regard, and I have no doubt it would show a very marked increase. How much of this is owing to the multiplication of psychologists and facilities for work as an indirect result of Canada's economic expansion in the last decade, and how much to the intrinsic growth of science itself, or the science in its public position, I don't know. I suspect, however, these are not true alternatives, but have both been necessary conditions.

In looking for some means to evaluate the condition today I made recently a rough and elementary examination of the volume of journal and other comparable publication by Canadian Psychological Association members. This was based on *Psychological Abstracts* for the years 1950 and 1951, during which there were a total of 120 entries for CPA members out of over 15,000 entries altogether. By comparison, APA members accounted for something over 4,000. However, of the 120 scholarly publications by CPA members, between one-third and one-half were by a limited number of CPA members resident in the United States or the United Kingdom, most of them permanently so. Thus against a biennial publication rate of APA members of about 50 per cent (that is, one publication per two persons over the two-year period), the rate of CPA members was 20 per cent, and allowing for the 8 per cent of non-resident CPA members having a publication rate of 100 per cent, the net CPA member rate was about 13 per cent, or one-fourth of the APA rate. You may object that APA and CPA membership conditions are not comparable, and this is probably so. If you exclude CPA associate members and student members from the reckoning, the number of scholarly publications by resident members reduces to 50, yielding a rate of between 25 and 30 per cent, or about one-half the overall APA member rate. Incidentally, of these 50 publications, I should estimate that about one-half were research reports or analyses. Since there are over 8,000 APA members and associates resident in the United States, and fewer than 200 CPA fellows and full members resident in Canada—a ratio of between 40 and 50 to 1—I suppose that taking the two countries as comparable samples one might reach such conclusions as that the arithmetic rate of increase in psychologists is of the order of three times that of economic productivity and four times that of population, while in turn publication activity is twice that of professional association membership, and varies almost with the square of general population.

At any rate, from such statistics the deduction is possible that our volume—I say nothing about the quality—of research, while probably increased substantially since the pre-war years, is considerably less than that characteristic of the United States. This, of course, is based on the assumption that journal and other comparable publication is fairly representative of research.

From the point of view of one whose job is to incite or support the development of research, or if for any other reason one is inclined to measure group status by research output, this conclusion might seem somewhat unsatisfactory. I think, however, that more properly it suggests the desirability of considering the distribution and organization of professional interests and facilities in Canada. There seems to me to have existed in the past a vague conception whereby an ideal consisted in a college or university professor who both taught and did publishable research, and whose status was accounted for by the former activity although his advancement probably depended on the latter. When this balance was not attained some deficiency was imputed. Thus the good college or university teacher has envied the productivity of the psychologist engaged in full-time organized research, while the research worker outside the academic walls has sometimes felt a loss of status in not being a teaching university member. With this conception has gone a tendency, in certain instances, I believe, to try to force the development of research facilities and programme in the essentially undergraduate teaching college, and to look down upon the college teacher who did not attempt systematic research.

I feel that this is a great mistake, and that we should recognize quite openly that there are several main types of desirable psychological activity, including general undergraduate teaching, research and research-oriented training, and professional practice and practice-oriented training. All three in due proportion are necessary and equally valuable, and should be equally rewarded and rewarding. A single individual is not likely, however, to be equally interested and effective in all or even in two. If he attempts more than one as his main focus of endeavour he is quite likely to end with an unsatisfactory compromise—unsatisfactory, that is, if we set really high, challenging standards of performance in each of these main fields. A number of you may have read a recent paper by Buxton of Yale University who attempts to make this point mainly as it affects the college teacher. This is essentially that some people are better at teaching, some at research, and rarely are the two combined in one; that it is unusual for a teacher who is doing a really first-class job with a full course load to have time for original research; and that there is enormous room for improvement in both the teaching and the research

jobs that have to be done, each of which is valuable and should not be done at the expense of the other.

A likely corollary of the foregoing is, I think, that since our numbers are not great we should not expect in Canada to see research facilities and programmes scattered evenly and uniformly across Canada proportionate only to regional population or economic resources. I would urge rather that we apply the military principle of the strength of aggregates, and seek to build up in the appropriate places specialized facilities and staffs whose particular competence one will not expect to find elsewhere. This remark applies, of course, to centres of professional training and of research, since almost by definition facilities for liberal arts and science teaching, including teaching of psychology to the undergraduate in relation to his needs and comprehension, must be spread across the whole land. With such teaching as a background, and on the assumption that many able psychologists will be acknowledged as having their aim focussed squarely and solely on doing this job superlatively well, I would then expect to see perhaps three or four centres wherein the bulk of our high-level professional training in clinical, educational, and industrial psychology was concentrated, or in which our experimental research-oriented psychologists and selected graduate students in psychology would have little or no other responsibility.

Many of you may feel that this is a rather commonplace observation, being in accord with what is tending to a considerable extent to be the case now. That it is tending to be the case so far as distribution of research is concerned is suggested by the fact that over the last four years Defence Research Board and National Research Council research grants and contracts for psychological and related studies have been assigned to the extent of over two-thirds of the monetary value involved to two institutions, and to the extent of 85 to 90 per cent, to five institutions, out of a total of thirteen institutions receiving such grants or contracts. However, the point is not simply that this is tending to be the case, but that the fact is open to acknowledgement, that such acknowledgement may be desirable as it clarifies the rôles of individuals and of institutions, and that the implications of this situation may need to be pursued rather more closely and fully.

Beyond the fact that one would simply not expect the majority of higher educational institutions to undertake systematic research on any significant scale or top-level professional training, it follows that staffs in a few institutions must be substantially free of undergraduate teaching loads to devote themselves to research programmes and to graduate training which thrives only in a pure research atmosphere, that schemes for supporting training for professional practice must not be hobbled by

provincial barriers, that research funds must likewise be concentrated where they will do the most good, and that scholarships must be available so that first-class graduate students can move freely from their province of undergraduate origin to that centre perhaps a thousand or two miles away where they can get the best and not merely the most conveniently located training. It follows also that the majority of institutions not attempting systematic research programmes have a special responsibility to detect, advise, and help select on the basis of some communicable standard those undergraduate students who may most profitably spend the next several years in another institution elsewhere in research or other professional training.

Deliberate concentration and development of first-class research facilities and staff in a few institutions should enable the application of the standard of design of research programmes which Marquis, in his presidential address to the American Psychological Association several years ago, suggested as calculated to extend most rapidly the limits of knowledge in those frontier fields where the need is greatest. Such programme design, he pointed out, involves a number of scientists working for several years on a central problem. An attempt is made to plan a comprehensive, integrated series of studies in relation to a particular set of concepts focussed on this problem. The formulation of specific and rigorous hypotheses which can be tested and which eventually enable adequate construction of theory and the development of new and useful techniques of direct observation and measurement, are characteristic features of good programme design of this kind. But this type of programme usually involves a minimum critical mass of scientists, time, and equipment, and cannot be done in small, unrelated particles.

I am not sure whether the division of labour which my remarks imply is something that comes about through natural force of circumstances and a common acceptance of these or whether it calls for some more deliberate agreement and formal implementation by the psychologists involved. I suspect it is apt to be the former, facilitated perhaps to some extent by the councils of those national bodies which can, and deem it wise to, influence the situation one way or another.

Having delivered myself of what some of you will perhaps regard as a few obvious and fairly well-integrated prejudices on the distribution and organization of research facilities in Canada, I should like to pass on to the other main subject-matter which I wish to discuss, namely the development of psychological research in problems of defence, and the relation of this to other and especially fundamental psychological research in Canada and elsewhere. These two—research directly applied to military problems and normal fundamental research—are of course related in

principle or in logical sequence, in that the former is to a great extent like a well from which you get your water for immediate use, while the latter resembles the sub-surface water-table which makes the well's long-term existence possible. In the organization of research facilities which, as I have pointed out, stemmed historically from the contribution which you and your colleagues made in wartime, this relationship has been recognized and provision made, not only for applied in-service research and development but also for support of basic research in universities in areas likely to have special significance for defence, and, through a separate organization, for support of free fundamental research or on occasion for investigations of an applied nature of special interest to government departments other than defence. The former of these has been developed through the Defence Research Board, and the latter through its elder scientific sister, the National Research Council.

At this point I wish to pause in order to make here a statement which I believe needs to be recorded, a statement of appreciation of the time, thought, and effort which a number of senior psychologists have given, in many cases without direct recompense but at some considerable cost to themselves, in advising upon and guiding through various committees, the scientific programmes sponsored by these two agencies. Without many hours of work on their part the degree of success which has been achieved in these programmes over the last few years would have been unattainable.

From the standpoint of national, as well as national defence interest, I don't know whether we have in these two organizations a reasonable balance of emphasis on various types of research or not. So far as fields of psychology are concerned, we are supporting work in almost all except clinical, which has received the least attention simply because the federal-provincial mental health programme has provided a separate means for promoting and assisting research and development in this area. With respect to the division between applied and basic research—a division which I find rather arbitrary at times, and which I should like to discuss further a little later on—I would estimate—and it is a little difficult here to know what units of measurement to use—that about one-fourth of the in-service work and 80 to 90 per cent of the research supported in universities under these two programmes is basic research, or perhaps one-half in all.

I have come to the conclusion—and this may be a somewhat surprising conclusion—that currently we are giving financial support in the form which this support can at present take, namely payment of direct, but not indirect costs of research, to most of the good research undertaken in universities which psychologists are able and willing to do. This takes

us back to the point that there is a distinct limit on the effective amount of research time and talent available. This can be increased only by augmenting the number of psychologists trained in research on the staffs of those universities where good facilities exist or can usefully be developed, and by allowing them major freedom from other duties. In the National Research Council committee with which I, and a number of you, have been concerned, we have discussed a step consistent with this in providing post-doctoral fellowships which would enable first-class research people to carry on work for at least a couple of years at a time without other major responsibilities. We have not yet been able to obtain unqualified approval and backing for such action, but I am optimistic about the prospect that we will.

Turning now more closely to defence research: in the last war most of you who made use of your professional training in the armed services did so in organizations which were built up in each service to help deal with the problems of manning and personnel selection which, after the first year or two of the war, came to be recognized as areas in which psychologists could be useful in an advisory capacity. This involved working close to administrative agencies engaged in selection of personnel and in certain instances extended into the region of training policies. A limited amount of research was done, mostly relative to the immediate problems which had to be solved. Looking back, I think one can say that on the whole psychologists were, after a visible hesitation at first, effectively employed. Researchwise, however, there were many possible areas of study in which they were not engaged, for the reasons that these were not yet recognized as important, or could not be touched for want of sufficient research staff to go around. It has remained for peacetime to review all the possibilities of application of psychological techniques, and following consolidation of the knowledge and experience gained in wartime, to experiment with trial studies in new areas.

I have found it useful—and the idea is certainly not original with me—to think of the major problems of human activity in defence in terms of the goal of manpower economy. In terms of this criterion there are a succession of forms or levels of action all tending to conserve human resources in defence but decreasingly economical in principle.

The first of these is the development and use of defensive techniques which do not depend on the composition and maintenance of armed forces at all, or which at any rate do not consist in the direct employment of physical force. The most successful defence is obviously one which is effective, but in which no blood is shed at all. This perhaps sounds a little other-worldly but is realistic in the sense that it points to all those means by which moral persuasion can be substituted for force—psycho-

logical warfare is the term perhaps most commonly used in this connection—and in which a psychologist, or any other kind of student of human behaviour, ought to be interested.

Next to this, there is a great group of problems which relate to the means by which men's jobs can be made as easy to perform as possible. The motif here is the reduction of the demands made upon an individual relative to the level of performance or output resulting from his behaviour. Under this heading may be placed studies of weapons and equipment with a view to making them easier to handle and use, simplification of methods and organization of work, investigation of environmental forces with the object of protecting the individual from them, analysis of operational stresses with the purpose of determining how they can be made easier to bear, and study of problems of morale, leadership, and human relations.

The foregoing implies trying to narrow the gap between what the individual is asked to do and what he can readily do by changing the job or the conditions under which it is performed rather than by altering the man. The next most economical step—economical that is in getting the most from the human resources available—is by improving in a direction conducive to better performance the existing attitudes, skill, or knowledge of the man. This leads mainly into studies of training: means by which more realistic and exact operational criteria for training can be got and standards set, improvement in the speed and quality of training, use of training aids and training devices, and the like. All of these have in common the intent to improve the quality of the men you have rather than to get basically better men.

Finally the least economical move—least economical in the sense in which I am using the term economy to apply to any situation where the number of men is limited—is in the field of selection and classification of personnel. It is least economical, of course, only in the sense that in terms of it alone you cannot transcend the capabilities characteristic of the manpower pool with which you commence, and that there is an inherent tendency, either because abilities are positively correlated or because there are certain common denominator characteristics, minimum levels of which are required in all jobs, for selection to be exclusive—to reduce the number available for final duty. You might ask, if this is the case, why so many of us were employed in selection of personnel during the last war, rather than in more positive or constructive undertakings? The answer is fairly plain, and I think twofold: first, that I have stated only one side of the case, in that insofar as abilities are *not* positively correlated, a sorting-out of people is profitable, and there is little point in putting into training or operations people for whom you cannot reduce

or ease the work-requirements sufficiently that they can meet them. Second, that *time* is an important consideration, and whereas under relatively peaceful conditions you have time to undertake research and development which may have greater long-term constructive value, in wartime you are more concerned with quick and substantial returns on immediately pressing problems, provided that these answers are not seriously inconsistent with the long pull.

In the programme of military psychology we have tried to set up, we have undertaken, at the risk of scattering our energies, but fortunately with direct and powerful assistance from many of you, work in all these major fields, and in relation to the needs of all three armed services and civil defence. The largest single portion of our effort is still in the area of personnel selection and classification, partly because this is the region in which our capabilities are best known to the armed services and to ourselves, and because it is here that wartime contributions by psychologists have been consolidated in permanent selection service organizations. However, as we gain experience, we are gradually extending, and expanding relatively, our research effort into the investigation of what I have previously described as the more economical means of conserving human resources in defence.

There are a number of more general observations, not very systematic, I am afraid, but lying on the surface of my thinking, which I should like to go on to make.

One of these is that in the kind of research programme I have been discussing there is little exclusive proprietary right, insofar as the human operational problems are concerned, on the part of any one academic field, but on the contrary, there is every advantage in the co-operative approach of a number of disciplines. Thus we have largely given up any prescriptive use of the term psychological research except to denote that work which is actually done by psychologists as such. We have made increasing use of the expression "human resources research" to denote the joint employment of the knowledge and techniques of psychology, education, sociology, psychiatry, physiology, statistics, and engineering in the study of problems before us. This does not, of course, mean that the psychologist attempts to be an engineer, or the sociologist to be a physiologist (although I should like to remark parenthetically that we have helped to foster two or three interesting hybrids, such as the psychologist with engineering training). It does not even mean necessarily that you have two or more persons from different fields working closely together as a team on the same problem, although to some extent that also has taken place. It means rather more obviously that the problems of enhancing the effectiveness of men in a military organization can be

solved more readily if the approach to these can be many-sided and comparative.

This inter-disciplinary approach is for defence purposes organized or built-in quite formally, since what I have been describing is in turn a building-block within a still broader research structure of sciences directed toward medical research, environmental protection in all its aspects, and operational research. The last is of some special interest in the present connection since, defined as the study of whole processes or operations with the object of making the most effective use of men and material at the lowest cost, it may include the evaluation of activities otherwise of interest to technical (including psychological) research, and necessarily in the study of tactics or other man-equipment combinations must take account of the human factor involved. The effect, so far as human resources is concerned, of this linkage with operational research has been to orient technical research in an increased degree toward operations in a theatre of war, and to influence it in the direction of critical, analytical, evaluative studies of personnel processes or operations.

One of the things which has struck me most fully has been the practical justification of much fundamental research in psychology, and especially experimental studies of human adult processes of sensation and perception, learning, motor function, and emotion and reaction to stress. I don't know whether these ever needed to be justified. Presumably, as fundamental studies they would not. Nevertheless, there were perhaps some of us fifteen or twenty years ago who may have had to go on the defensive occasionally when it came to explaining or accounting for such basic experimental studies to the severely utility-minded individual. Such is far less if ever the case now, at least in the military field, because it has been appreciated that you cannot put man into a critical, competitive, and complex situation under stress wherein he must learn to perceive and react accurately and quickly unless you understand as fully as possible what he can do and help him to do it better—that is, you cannot afford to ignore all this if you expect to win battles. And to have this kind of knowledge to apply you have to acquire it in the first place, often by long-term and broad basic research.

This same appreciation which is now applicable to the fruits of differential and experimental psychology will, I think, gradually extend in future to social psychology and related social science disciplines which we have been beginning to apply to defence studies, but which being less "scientific" in their aura or appearance are less readily accepted at once. This is, I think, only a question of time and competent effort.

The overall feature of this situation which perhaps makes it most fruitful for or accessible to psychological study is that for purposes of

war any and every aspect of the individual's living and functioning—in this case that of the young adult male—is important if he is to be made and kept effective. He is in the service day and night, on duty and off. It is to a great extent a self-contained, bounded situation. An abnormal one, I agree, and a regrettable one, regrettable, that is, in its basic necessity, but one which facilitates a systematic approach and one almost unlimited in its topical variety.

One other phase of defence human resources research which I believe is of particular interest is the extent to which it implies and may serve to promote knowledge of the characteristics of the Canadian population, by age, region, cultural features and education, physical traits, abilities, and skills, and in all the many ways in which a nation's people can be known and described. We have in my opinion learned a good deal about the characteristics of the Canadian population in the last ten years that we did not know, or did not know as precisely or as well, before, and I hope and expect that we shall be able to extend this knowledge much further in future.

In conclusion, and I fear that I shall not reach a conclusion, although I promise you that I shall come to an end, I should like to add a word on the subject of applied versus basic or fundamental research, and on the characteristics of organized research. Several years ago the APA held under the auspices of the Division of Military Psychology a symposium on the status and the advantages and disadvantages, from the psychologist's point of view, of organized research, by which I presume was meant full-time research planned as to programme and with certain definite goals in mind, in universities, defence or other government laboratories, and voluntary or private agencies. They concluded that such research was apt to be as stimulating and as productive of representative work of scientific repute as under traditional academic auspices, and that it held certain advantages in that amenities, such as clerical or technical help, equipment, and facilities for travel, were apt to be better. I hope that this is true. I am confident that it can be, provided that we learn what are the right conditions under which it will flourish. One of these conditions has sometimes been alleged to be, at least in certain settings, the balance between applied and basic research, although as these terms are used in the field of psychology, I confess that definite ideas which I once had as to what constituted each have become somewhat fuzzy, or perhaps the importance of an exact distinction between the two has in my mind declined. This distinction lies in principle, I suppose, mostly in the purpose which one has in mind at the time, whether one is seeking an immediate practical solution or is concerned merely with the systematic exploration of a field of ignorance. In the case of so-called applied research

one is more likely to require an answer in order to make a decision, and the area of reference is apt to be narrower because it corresponds with some region of executive control or influence. In the case of basic research one is not concerned with an essential decision, and the apparent degree of generalization or abstraction may be greater. But there need be and should be no difference in the method or quality of the research, and unless one is working in an exact scientific field, which I think is not true of the study of human behaviour, I would aver that truth is frequently arrived at by accumulation of evidence from a variety of sources. Suppose one undertakes a study of a work-unit with the object of determining whether attitudes to certain stated features of the work-situation affect performance, or investigates the sources of human error in the manipulation of the controls of a machine, or analyzes data bearing on the question of whether battle stress affects ability to do physical work, or examines the comparative personal history of recruits who adjust well or poorly to military discipline, or studies the learning process involved in acquiring skill in morse code. These are all applied researches, in that they are done for practical reasons, and with the object of facilitating executive decisions by providing a reliable basis of generalization about the phenomenon in question which can be used in connection with that decision, but I would argue that at the same time they might very well constitute contributions to general knowledge. And, if one adds to this the case where, as has been tending to happen, a long-term study is called for—like the investigation of anxiety and panic in relation to large-scale disaster, or the effect of climatic extremes on psychological processes, or the study of intercultural relations within a nation, or the relation of stimulus magnitude to the perception of colour—before a policy or an equipment programme can be endorsed with confidence—then I am not sure that I can tell just where one kind of research ends and the other begins. It would almost appear to depend on whether somebody had thought of a particular reason for it in advance or not. In fact that is the point: it is a question of your attitude. I think that what is plain is this: the usefulness of all good research, and I am now speaking of psychological research or of human resources research, has gradually been becoming more apparent. As this conviction grows the fine distinction between applied and basic research breaks down somewhat and while the difference is quite evident at the extremes, what you see in the middle of the scale is only whether the research is short or long-term, narrower or broader in its possible implications. An organized programme such as a defence research programme or any other programme having national or other socially significant aims is likely to call for all of these, simply because the questions that may usefully be asked are of various

kinds. The problem is merely to determine what at any given time is the optimal balance in this programme. I believe that this is a good thing, because it permits one to think of research in our field as a greater unity, and facilitates communication and understanding among all classes of research workers, which is much to be desired.

MOTIVATIONAL AND EMOTIONAL CHARACTERISTICS OF "BRIGHT" AND "DULL" RATS[1]

WILLIAM R. THOMPSON and DALBIR BINDRA[2]
McGill University

Introduction

Strains that are "bright" and "dull" in maze-running have been selectively bred both by Tryon (8) and Heron (4). Although this work has indicated that the ability of rats to perform in mazes follows an inherited pattern, the actual nature of this pattern is virtually unknown. There have been two main reasons for this fact: first, as Hall (3) has pointed out, homozygous "bright" and "dull" strains have not been produced by selection experiments, this precluding the possibility of any reasonable genetic analysis; and secondly, the characteristic studied, that is, maze-running, is one of a highly complex nature, involving, besides intellectual aspects, motivational, emotional, and physical aspects. This fact has been clearly demonstrated by Searle (7) who has presented evidence that Tryon's "bright" and "dull" strains have entirely different constellations of traits and abilities, though they are relatively homogeneous within each group. Using Q-technique, Searle computed correlations between each rat and every other rat with respect to 30 behavioural variables. The average intercorrelation for "bright" rats was found to be 0.587, for "dull" rats it was 0.533, and for "bright" and "dull" –0.188. Accordingly, there are grounds for concluding that the two strains bred by Tryon have entirely different kinds of make-up; but at the same time, the correlations within each group are not as high as might be expected with complete homozygosity.

Consequently, two major tasks face the experimenter in this field. First, either the development of homozygous strains by appropriate inbreeding, or the use of pure lines already available (for example, mice). Second, careful analysis on the phenotypic level of the behavioural trait under study. Thus if we wish to study the genetics of intelligence, emotion, or motivation, it will be necessary to isolate each, so that, ideally, the two strains used are at opposite poles of the distribution of the trait of primary interest but are randomly distributed with respect to any other unitary trait.

At the McGill Laboratory, a selective-breeding experiment has been undertaken by Wrigley and Thompson (9). This has been designed to take account of both of the problems outlined above. Up to date, four generations have been bred selectively on the basis of performance on the Hebb-Williams maze. The latter is a simple maze with adjustable

[1]This research was supported by the National Research Council of Canada.
[2]The authors are indebted to Messrs. M. P. Hyatt, M. Ripsman, L. M. Solomon, and E. B. Van Zaig for their valuable assistance in carrying out the experiment.

Can. Jour. Psych., 6, 3 (1952).

partitions which allow a great diversity of problems to be used. Twenty-four such problems have been standardized (6), the second 12 being mirror images of the first 12, thus furnishing a check on reliability. Training is carried on until each animal is thoroughly familiar with the situation. In the last generation tested (F3), there appeared a clear separation between animals of "bright" ancestry and those of "dull" ancestry. The difference was more marked in the case of males. The next generation has already been bred and will be tested shortly.

Now, in view of the ambiguities of previous experiments which we have discussed above, it seemed desirable to examine more carefully at this point, before proceeding with selection, differences characterizing "bright" and "dull" rats other than performance on the Hebb-Williams maze. This constitutes the purpose of the present paper. Three parameters were chosen for study: motivational (food-eating behaviour), emotional (defecation and urination), and physical (weight).

The Experiment

Subjects

Thirty-three animals, 15 "bright" and 18 "dull," of the reserves of the McGill strains were used in the experiment. Nineteen of these were albino, and 14 were hooded. At the time of testing, the rats were approximately 100 days of age.

Method

Test (a). Modified Hall "open-field" test (2). The apparatus consisted of a large metal box (24″ x 18″ x 16″ high) with no bottom. It was placed, at each testing, on a floor of glass which could be cleaned after each animal had been run. Procedure was as follows: each S was put on a 23-hour deprivation schedule, and each day, for ten successive days, was placed in the box alone for ten minutes with a small dish of semi-liquid food. After testing, each animal was allowed supplementary feeding, males 31 grams, females 25. On the tenth day, a disturbing element was introduced in the form of a metronome clicking continuously (150 clicks per minute) during the ten-minute test period. The following records were taken: (1) amount eaten, (2) time spent eating, (3) drop in amount and time on the tenth day, (4) defecation and urination.

Test (b). Timidity test. This test was used previously by Bindra (1) in studying hoarding behaviour. Each animal was kept in a small living cage, one rat per cage, for three days and deprived of food for 23 hours between the third and fourth day. On the fourth day, the door of the cage was left open, and the rat allowed access to food by means of a narrow plank (5″ x 50″). The cage and the food were both three feet

above the floor. The food tin was placed 36 inches from the entrance of the cage. Timidity scores were in terms of the time taken by each animal to reach the food from the time the door was opened.

Results

Test (a). 1. Amount eaten. Curves based on mean amounts eaten by "bright" and "dull" rats over the ten days are presented in Figure 1.

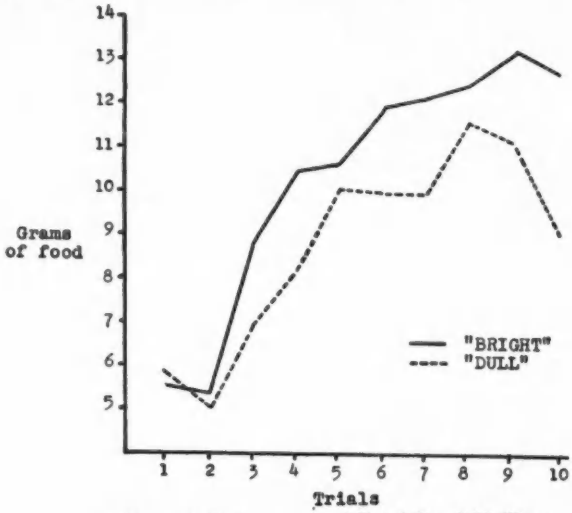

FIGURE 1. Eating curves of "bright" and "dull" rats.

It is clear from the curves that while both groups start out initially at about the same level, the "bright" group quickly gains the advantage in terms of amount eaten. Further, the differences, if taken in three groups of three trials each, become progressively greater from the first to the ninth day.

The differences illustrated in Figure 1 hold within each of the subgroups of male, female, hooded, and albino, indicating that the total differences are not due to a preponderance of albinos or females in the "dull" group.

In spite of these facts, however, no significant difference can be shown statistically between the "bright" and "dull" with respect to mean amounts of food eaten over the ten days taken together for each animal ($P > .40$).

On the tenth day, when the metronome was introduced, it will be noted from Figure 1 that, in general, both groups show a drop in eating, but the "dull" more than the "bright." This difference also holds between each of the "bright" and "dull" subgroups, with the exception of the

hooded animals. These showed a gain in trial 10, on the average, this being greater in the case of the "dull" hooded. "Bright" females also gained slightly.

However, it is important to notice that of the 15 "brights," 11 showed a drop on the tenth day, and four showed a rise; while of the "dull," seven only dropped, and 11 rose. The difference between these proportions approaches significance (Chi-square = 2.64, $P < .15$). It is therefore difficult to state categorically which group was more disturbed by the metronome.

Rank order and biserial correlations were calculated between eating amount, weight, and "brightness." These are as follows: between eating amount and "brightness," 0.489; between eating amount and weight, 0.741; between "brightness" and weight, —0.05. Since the "dull" were found to be slightly heavier on the average, the correlation between eating amount and "brightness" rises to 0.783 when weight is partialled out.

2. *Eating time.* The main results for eating time correspond closely, as might be expected, to those for eating amount. The curves for the two groups, based on total means each day for the ten days, are presented in Figure 2. As before, the "brights" spend a longer time eating than the "dulls" in almost every trial, except the initial one. Further, the differences increase sharply through Trials 1 to 9.

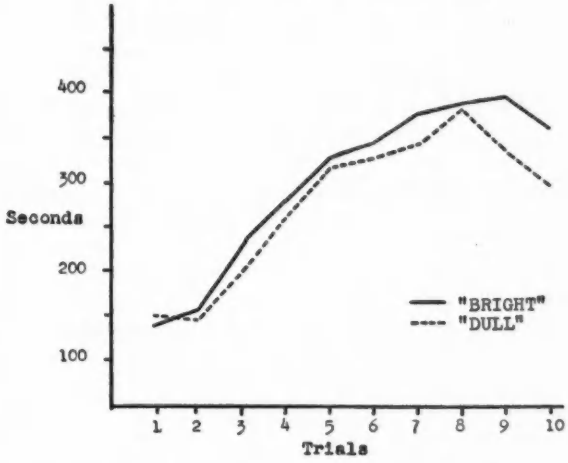

FIGURE 2. Eating curves of "bright" and "dull" rats.

However, in the subgroups, there is a distinct overlap in the eating curves of "bright" and "dull" females, and a reversal in the case of "bright"

and "dull" albinos. That there is a disparity between eating amount and eating time may be due in part to errors of measurement, and in part to actual differences in rate of eating. The correlation between the two over the ten days is 0.782.

Again, there is no significant difference between the total eating times of the "bright" and "dull" groups ($P > .40$).

As shown in Figure 2, the drop in time spent eating on trial 10 is sharper for the "dull" than for the "bright" rats. This difference does not hold in the case of hooded animals. Of the latter, the "dull" gained on the average in trial 10, while the "bright" lost. "Dull" males showed a somewhat smaller drop than "bright" males, but "dull" females a shorter one.

Correlations between eating time, "brightness," and weight are as follows: between eating time and "brightness," 0.313; between eating time and weight, 0.452; between "brightness" and weight, –0.050. The correlation between eating time and "brightness" rises slightly to 0.376 after weight has been partialled out.

3. Defecation and urination. The frequency of defecation is not significantly different between the "bright" and "dull" groups, either over the ten trials (Chi-square = 0.34, $P > .50$), or over the first three trials only (Chi-square = 0.53, $P > .30$). The "bright" animals, however, urinated more frequently over the ten days than the "dull" (Chi-square = 3.96, $P < .05$), though not over the first three days (Chi-square = 1.48, $P > .20$).

Males and females do not show a significant difference in defecation (Chi-square = 0.09, $P > .70$), but females urinate more over the ten trials (Chi-square = 14.96, $P < .001$). Albino animals defecate significantly more than hooded (Chi-square = 10.28, $P < .01$), but these two groups do not differ in urination frequency (Chi-square = 0.038, $P > .80$).

In general, as shown in Table I, defecation declines through the ten trials for both "bright" and "dull" animals. Urination, however, remains more constant.

TABLE I

FREQUENCY OF DEFECATION AND URINATION FOR "BRIGHT" AND "DULL" RATS

	Trials					
	1–3		4–6		7–9	
	D[1]	U[2]	D	U	D	U
"Bright"	4.8	10.8	2.4	11.2	1.5	9.4
"Dull"	4.7	8.6	3.7	7.0	2.0	9.5

[1]D = defecation
[2]U = urination

Summarizing the results of Test (*a*), it seems clear that there is a definite difference between the curves of "bright" and "dull" rats in amount eaten and time spent eating over the ten daily ten-minute trials. The difference increases from the first three to the last three trials. There are no statistically significant differences, however, between the two groups in total scores over the ten days together, either in eating amount or time. Nor are there marked differences in weight and emotionality between the "bright" and "dull" animals.

Test (*b*) *Timidity.* Since there was considerable spread in scores on this test (seconds before reaching food), the animals were put in rank order and divided into two groups, timid and not timid, the dividing point being the median rank-order score. The numbers of "bright" and "dull" rats falling into each group were then compared. No significant differences were found (Chi-square = 2.01, $P > .10$), though there was a slight tendency for the "dull" to be more timid than the "bright." No differences appeared between hooded and albino rats (Chi-square = 0.52, $P > .30$), but females did seem to be somewhat more timid than males, though not significantly so (Chi-square = 2.50, $P > .10$).

Discussion

In general, the results indicate that the "bright" animals may be more "food-driven" than the "dull." This supports the findings of Kruse (5), and Searle (7). However, the difference in this case seems to be not so much a relative one, such as Searle found with Tryon's "bright" strain, as an absolute one. This difference does not decline with adaptation to the testing situation, but increases. This affords one indication that it is not dependent on emotional behaviour, which declined through the ten days. A second indication is given by the fact that there are no significant differences in emotionality between the two groups. This conclusion is supported by the relative reaction of the "bright" and "dull" animals to the disturbing stimulus on trial 10 of Test (*a*), their defecation and urination scores in the same test, and also by their relative timidity on Test (*b*). Apparently, then, the two strains are rather different from those of Tryon in this respect. Searle found Tryon's "bright" strain to be more emotional in an open-field situation (7), and Kruse (5) also found previously that an interruption during eating caused more disturbance among Tryon's "brights" than his "dulls." Further, the difference in "brightness" is also independent of weight, although it seems that the heavier animals of both groups eat more.

Consequently, it appears reasonable to conclude that "intelligence" in rats as measured by the Hebb-Williams maze procedure represents a parameter correlated with food-drive, but orthogonal to emotionality and weight. It would seem feasible, in the event that this generalization

holds true in the next generation,[3] to cancel out this difference by appropriate selective breeding, in order to isolate more completely the purely intellectual differences. When this has been done, it should then be possible to breed for various sublines, for example, "bright," emotional, low food-driven, as opposed to "dull," unemotional, highly food-driven. In this way, it should eventually be possible to study the genetics of these traits either singly, or in combination.

Summary and Conclusions

The purpose of the experiment was to examine the physical, emotional, and motivational characteristics of the "bright" and "dull" strains bred selectively at McGill on the basis of performance on the Hebb-Williams maze.

Thirty-three animals of the reserve stock (15 "bright," and 18 "dull") were used in the experiment.

Two tests were given to the animals, (*a*) a modified Hall open-field test, and (*b*) a timidity test. Weight was also recorded.

On the basis of scores obtained by the "bright" and "dull" groups in amount of food eaten, time spent eating, percentage drop due to the introduction of a disturbing stimulus, defecation and urination (Test (*a*)), and timidity (Test (*b*)), it was concluded that, while the "bright" were probably more food-driven than the "dull," the two groups were not different in either emotionality or weight. It was proposed that breeding might be arranged so that the motivational differences might be eliminated in succeeding generations.

References

1. Bindra, D. "What Makes Rats Hoard?" (*Journal of Comparative and Physiological Psychology*, 41, 1948, 397–402).

2. Hall, C. S. "Drive and Emotionality: Factors Associated with Adjustment in the Rat" (*Journal of Comparative Psychology*, 17, 1934, 89–108).

3. Hall, C. S. "The Genetics of Behaviour" (In S. S. Stevens (ed.), *Handbook of Experimental Psychology*, pp. 304–29. New York: John Wiley & Sons, Inc., 1951).

4. Heron, W. T. "The Inheritance of Maze Learning Ability in Rats" (*Journal of Comparative Psychology*, 19, 1935, 77–89).

5. Kruse, M. "Food Satiation Curves for Maze-bright and Maze-dull Rats" (*Journal of Comparative Psychology*, 31, 1941, 13–21).

6. Rabinovitch, M. S. and Rosvold, H. E. "A Closed-Field Intelligence Test for Rats" (*Canadian Journal of Psychology*, 5, 1951, 122–8).

7. Searle, L. V. "The Organization of Hereditary Maze-brightness and Maze-dullness" (*Genetic Psychology Monographs*, 39, 1949, 279-325).

8. Tryon, R. C. "Genetic Differences in Maze Learning in Rats" (*National Society for the Study of Education, 39th Yearbook*, Vol. III, 1940).

9. Wrigley, C. F., and Thompson, W. R. "The Inheritance of Learning Ability in Rats" (*to be published*).

[3] A subsequent experiment measuring the food intake of "bright" and "dull" rats of the next generation, (F 4), has given results similar to those reported above. In this case, however, the "dull" animals ate greater absolute amounts in most of the ten trials. But they were also definitely heavier than the "brights." With weight equalized, "bright" rats ate relatively more in all but three of the ten trials.

THE DEPENDENCE OF BINOCULAR FUSION ON TIMING OF PERIPHERAL STIMULI AND ON CENTRAL PROCESS

2. Asymmetrical Flicker

C. H. BAKER
Defence Research Board, Toronto

THE EXPERIMENTS reported in this part have a neurological emphasis and, accordingly, their introduction is concerned with the consideration of certain neurological facts and theories. These data are separately presented below and later combined to form the hypothesis which the experiments in this part were designed to test.

CHARACTERISTICS AND TYPES OF OPTIC NERVE FIBRES

From studies of the electroretinogram and the optic nerve discharge, considerable information has been gained concerning the activity of the sensory impulses which are initiated by retinal stimulation and pass back to the brain. Retinal discharge frequency produced by isolated flashes of light is a function of intensity of stimulation. It is increased by raising stimulus intensity. Also, it is increased by extending the flash duration if such duration is shorter than the latent period of the sense cells. Intensity of stimulation, too, when raised, shortens this latent period and results in an increase in the number of impulses discharged per flash (5).

All sense cells do not act alike with reference to such changes in stimulus intensity. Studies of vertebrates by Bartley (5) and by Hartline (8, 9) have shown that there are three different types of discharge upon retinal stimulation. Some 50 per cent of the fibres (the On-Off or Y type) discharge with a rapid burst of impulses at the onset or increase of stimulation. These impulses gradually decrease and disappear, only to burst into activity again when stimulation is reduced or removed. About 20 per cent of the fibres (the Maintained or X type) rapidly discharge at the onset or increase of stimulation, and then discharge at a steady but lower frequency during the remainder of the stimulation period. The remaining 30 per cent of the fibres (the Off or Z type) discharge only when stimulation is reduced or removed. Hartline was able to record discharge frequencies from afferent fibres just before their collection into the optic nerve. He found here, too, definite "Off" and "On" discharge patterns in single fibres.

Finally, Bartley (4) has shown that "the cortical record follows the retinal in its essential changes due to varying stimulus conditions . . . the mechanisms of the 'On' and 'Off' effects appears to be separated throughout the pathway."

Bartley has proposed that the three modes of fibre response may be due to any one of three causes: (1) there are three essentially different

CAN. JOUR. PSYCH., 6, 3 (1952).

types of sense cells, or (2) the diversity originates in the retinal layers between the sense-cell and the ganglion layer, or (3) there are functional differences between ganglion cells.

Crucial evidence which would lead to the acceptance of any one of these alternatives is lacking except in the case of the scallop. Hartline found two sense-cell layers in the scallop eye, the cells in one discharging to the onset or increase of stimulation and in the other to stimulus reduction or removal. This evidence suggests then, that in the vertebrate eye alternative (1) is to be favoured. The suggestion has been accepted by Morgan (12) who writes, "We find, then, that there are three distinct types of fibres in the optic nerve."

Latency of the Optic Nerve and Retinal Discharge

Morgan has pointed out that records of the optic nerve discharge indicate that impulses set up by a flash of light do not appear at precisely the same time as stimulation occurs, and are spread out longer in time than the flash. He believes that such a dispersion of impulses in transmission to the brain is brought about by the facts that: (1) all neurones of the retina do not respond to the stimulation at the same instant, (2) synaptic latencies are always somewhat variable, and (3) since nerve fibres vary in size, transmission in the optic nerve is of different speeds in different fibres (12).

The Alternation of Response Theory

When studying the retinal potential of the frog, Granit and Therman (7) found that at high frequencies of stimulus intermittence, retinal and optic nerve discharges occurred to only every second flash. At lower frequencies responses to successive flashes alternated in size (that is, large responses alternated with small responses). With still further reduction of intermittence frequency the responses were similar though smaller in size, and synchronous with the stimulus intermittence frequency.

Bartley (6) has confirmed these findings and suggests they indicate that some fibres concerned require longer than the flash interval to recover from stimulation. If some groups of fibres require more time than allowed by certain intermittence frequencies, then "reactivation of the eye by successive flashes would require the alternate functioning of parallel units serving the same retinal areas." As the period between the onset of successive flashes is lengthened, more and more "elements" are picked out by the alternate flash. "Finally, each successive flash is evoking equalized responses, but the elements responding to one flash are not those responding to the previous one."

From the above facts and theories it can be argued, then, that some

groups of retinal receptors and their fibres can respond to greater stimulus intermittence rates than others, that is, can transmit each flash as discrete events rather than as fused (for instance, receptors requiring long latent periods would transmit two successive flashes as one, rather than as two, as would be the case with those requiring shorter latent periods). Said in another way, the more nearly instantaneously the retinal receptors and their fibres are stimulated, the sooner some groups of receptors will be ready for reactivation (and so can respond to instantaneous stimulation), in contrast to the result when stimulation is slightly spread out in time.

Aim of Present Study

The aim of the present study was twofold. First the plan was (Experiment 8) to test the deduction made above by altering the manner of exposing and occluding a retinal stimulus, that is, not only to use Sherrington's plan (13), and that employed in Experiment 1 (1) for "progressive" control of stimulation across the area concerned in each retina (by a revolving sector disk), but also to provide for "instantaneous" control of stimulation over the retinal area (by illuminating with a neon source that could be cut off or on "instantaneously").

The validity of the above deduction being determined, then, (by demonstrating that the "instantaneous" method permitted higher critical flicker frequencies than the "progressive") it would be safe to conclude that neural discharge frequencies initiated by the two methods of stimulus occlusion are dissimilar for similar intermittence frequencies.

Presuming that the above experiment was successful, then, the plan was (Experiment 9) to test the major hypothesis of this part, which is identical with that of Part I (1, 2), but in a different manner. If a central process is involved in binocular fusion (as it is – Part I), then the critical flicker frequency, when "instantaneous" stimulation is applied to one fovea while "progressive" is applied to the other, will be somewhere between that brought about by the method of binocular "instantaneous" retinal occlusion (with many fibres being ready for reactivation by successive stimuli) and that brought about by the method of binocular "progressive" occlusion (with fewer fibres being ready for reactivation by successive stimuli).

Experiment 8

This experiment, without its attendant hypothesis, was separately reported in this journal last year (3). All differences between the "instantaneous" and "progressive" modes of stimulus occlusion for each subject, brightness, and degree of synchrony, were statistically significant. (Three

differences were significant at the .05 level of probability, nine at the .02 level, 34 at the .01 level, and 29 at the .001 level.)

It was concluded from Experiment 8 that, in order for an intermittent brightness source to be just perceptible as flicker at the fovea, the rate of intermittence can be greater if the source is instantaneously occluded than it can be if it is progressively occluded. Said in another way, the receptor and optic nerve discharge frequencies for the "instantaneous" and "progressive" methods of stimulus occlusion are dissimilar at similar stimulus intermittence rates.

Experiment 9

It is apparent from Experiment 8 that it is possible to vary the frequency of retinal and optic nerve discharges by varying the method of stimulus occlusion between "instantaneous" and "progressive." By "instantaneously" occluding the stimulus source (and hence the foveal image) it was demonstrated that retinal and optic nerve discharges more nearly reflected the discrete stimulus intermittence frequencies than was the case for "progressive" stimulus occlusion, that is, the eyes under the "instantaneous" condition could be said to have greater resolving power in time.

It is argued, therefore, that if central processes are active in combining and integrating the neural impulses from the respective retinas to result in binocular single vision, then the critical flicker frequency which is the result of "instantaneous" stimulation to one eye with synchronously presented "progressive" stimulation to the other eye will be somewhere between the dissimilar binocular frequencies initiated by "instantaneous" and "progressive" frequencies found in Experiment 8.

Method

Again the upper neon ("instantaneous") sources and the lower neon ("progressive") sources were employed. These sources, as in Experiment 8, were superimposed by, and regarded through, the upper set of flexible prisms. To obtain the condition of "instantaneous" stimulation of one retina with "progressive" stimulation of the other retina, a slightly lower set of flexible prisms was used to superimpose the left upper ("instantaneous") source and the right lower ("progressive") source.

Because of the small differences which were expected in critical flicker frequencies between the various conditions, ten readings were required at each condition, rather than the usual six. Five readings were taken using the "instantaneous" method (top prisms), followed by five using the "progressive" method (top prisms), and then, by slightly dropping the head to permit vision through the bottom prisms, five readings were taken using the combined condition ("instantaneous" to one eye with

"progressive" to the other, called the I-P condition). This process was immediately repeated and so resulted in ten readings at each condition.

It was necessary, in order to test the hypothesis, to perform this experiment at only one degree of synchrony, and the completely synchronous condition was arbitrarily chosen. Again readings were taken at each of the three principle brightness levels employed in the other experiments.

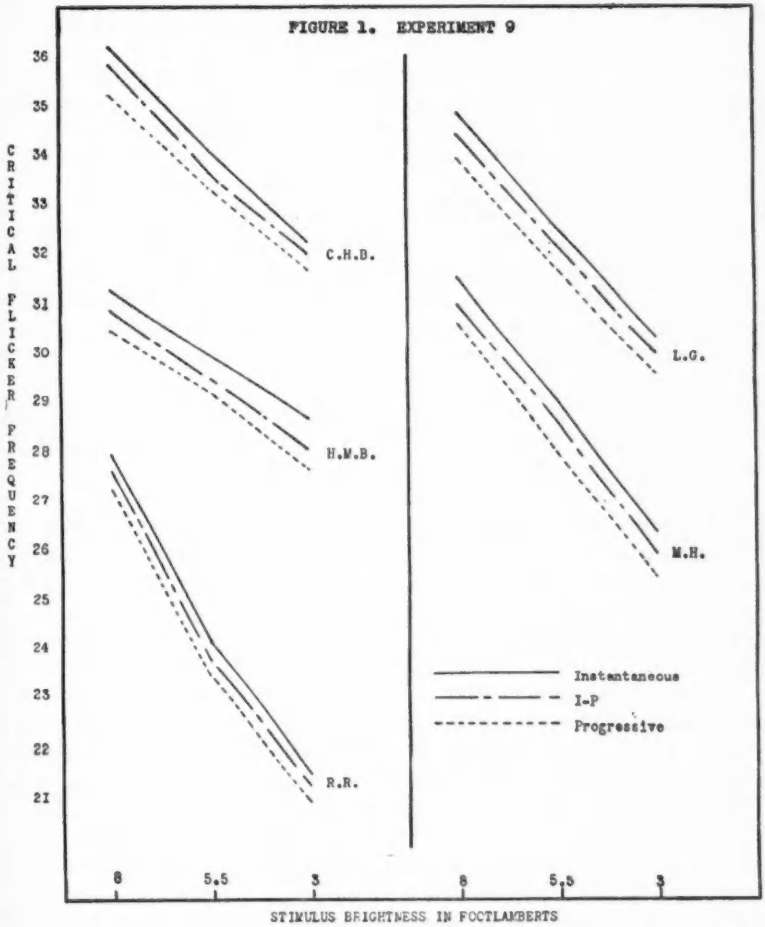

FIGURE 1. Showing the "instantaneous," "progressive," and "instantaneous-progressive" (I-P) critical flicker frequencies for the binocularly synchronous condition at three brightness levels, for each of five subjects.

Results

The results of Experiment 9 are graphically shown in Figure 1. All differences between the "instantaneous" and the I-P condition, and between the I-P and the "progressive" condition were statistically tested for significance, for each subject and for each brightness level. (Three differences were significant at the .01 level of probability and 27 at the .001 level.)

Conclusion

By stimulating one eye "instantaneously" and the other eye "progressively" the critical flicker frequencies yielded confirm the hypothesis that central factors are active in binocularly fusing neural impulses initiated by stimulation of the respective foveas.

Discussion (Part 2)

One of the main findings in these two experiments is that, in order for an intermittent brightness stimulus source to be just perceptible as flicker at the fovea, the rate of intermittence can be greater if the source is "instantaneously" occluded (that is, for the whole area at once) than it can be if it is "progressively" occluded (that is, by a sectored disk moving with a given direction and rate). It is interesting to note that Henry (10) used an electronic apparatus with neon stimulus sources and reported that he found flicker fusion rates to be slightly higher than those reported by other investigators using the sectored disk technique. Apparently he attached no significance to this finding.

The relative lengths of time necessary to completely occlude the apertures employed in these experiments by the "progressive" and "instantaneous" methods respectively still remain to be determined. Calculations show that a period of .002 seconds is required to completely occlude the ¾″ diameter apertures at an intermittence rate of 35 cycles per second, using the sectored disk. Information concerning the decay time of neon, however, is difficult to obtain. A personal communication from the neon bulb manufacturers stated that they did not know the decay time but that it was "extremely fast." An observation by Miles (11) is pertinent here. Miles used neon in flicker studies and reported that the neon light "could be turned completely on and completely off for intervals of .00001 seconds." If this can be taken as a correct figure, then a stimulus time interval of .00199 seconds makes a difference in flicker fusion intermittence rates of approximately .75 flicker cycles (the approximate average difference between "instantaneous" and "progressive" flicker rates for all subjects). It would seem, then, that such a comparative method

offers an approach to the study of retinal receptor latency periods. But such a study would be in the realm of neurophysiology and has not been further pressed.

The results of both Experiments 8 and 9 offer additional evidence of the validity of the alternation of response theory. And this evidence is directly in harmony with certain conclusions drawn from experiments in Part 1. Assuming the alternation of response theory to be correct, as it seems to be, then the results given above indicate, once again, that the central factors active in effecting binocular vision can perform their task *when non-corresponding retinal points are stimulated,* for, within any fovea, the receptors (corresponding points) alternate in responding to intermittent stimulation.

But the broad general conclusion from these experiments is that a central process is active in combining and integrating neural impulses from the respective foveas. For when the neural impulses initiated at the respective foveas are of dissimilar frequencies (that is, where stimulation is asymmetrical), the resultant flicker frequencies are somewhere near the mean of the values which result from the flicker frequencies yielded by either of the neural impulse frequencies considered alone.

In Part 1 evidence was obtained of synchronously timed stimuli (and resultant neural impulses) reinforcing one another in sensation, and of alternately timed stimuli (and resultant neural impulses) damping one another in sensation. These results were taken to be a refutation of Sherrington's Section I argument (13, 14) for independence. The experiments above (Part 2), on the other hand, are completely in agreement with those reported in Section II of Sherrington's experiments, in which he found that asymmetrical forms of intermittent stimulation resulted in: "Very considerably mutual interference between reactions initiated at the corresponding areas . . . which . . . might be evidence of mutual interference between purely physiological processes initiated at the corresponding spots of the right and left retina" (14).

Sherrington concluded from his symmetrically timed experiments (Section I) that no cerebral process was involved when both eyes are used. Although his asymmetrically timed experiments (Section II) suggested such interaction, he believed that his symmetrically timed experiments disproved such a suggestion. Therefore, he reasoned, visual fusion is wholly a "psychic synthesis."

The experiments reported here (Parts 1 and 2) have refuted Sherrington's Section I findings, and have confirmed his Section II findings. Therefore, the weight of evidence, from both parts, suggests that binocular fusion is a result, in part at least, of "purely physiological processes"

initiated at the respective foveas. Obviously, then, the study of such processes is more within the province of the physiologist than the psychologist.

Conclusions (Part 2)

1. A central process is active in combining and integrating stimulations initiated at the two foveas into a single binocular percept.

2. The central process operating to bring about binocular fusion can be demonstrated when non-corresponding retinal points are simultaneously stimulated.

REFERENCES

1. Baker, C. H. "The Dependence of Binocular Fusion on Timing of Peripheral Stimuli and on Central Process. 1. Symmetrical Flicker" (*Canadian Journal of Psychology*, 6, 1952, 1-10).

2. Baker, C. H. "The Dependence of Binocular Fusion on Timing of Peripheral Stimuli and on Central Process. 1. Symmetrical Flicker (*continued*)" (*Canadian Journal of Psychology*, 6, 1952, 84–91).

3. Baker, C. H. and Bott, E. A. "Studies on Visual Flicker and Fusion. II. Effects of Timing of Visual Stimuli on Binocular Fusion and Flicker" (*Canadian Journal of Psychology*, 5, 1951, 9–17).

4. Bartley, S. H. "Temporal and Spatial Summation of Extrinsic Impulses with the Intrinsic Activity of the Cortex" (*Journal of Cellular and Comparative Physiology*, 8, 1936, 41–52).

5. Bartley, S. H. "Some Factors in Brightness Discrimination" (*Psychological Review*, 46, 1939, 337–58).

6. Bartley, S. H. *Vision* (New York: D. van Nostrand, 1941).

7. Granit, R. and Therman, P. O. "Excitation and Inhibition in the Retina and in the Optic Nerve" (*Journal of Physiology*, 83, 1935, 359–80).

8. Hartline, H. K. "Impulses in Single Optic Nerve Fibres of the Vertebrate Retina" (*American Journal of Physiology*, 113, 1935, 59–60).

9. Hartline, H. K. "The Response of Single Optic Nerve Fibres of the Vertebrate Eye to Illumination of the Retina" (*American Journal of Physiology*, 121, 1938, 400–15).

10. Henry, F. "An Electronic Apparatus for Testing Fatigue by the Visual Flicker Method" (*Journal of Experimental Psychology*, 31, 1942, 538–43).

11. Miles, P. W. "Flicker Fusion Fields. II. Technique and Interpretation" (*American Journal of Ophthalmology*, 33, 1950, 1069–77).

12. Morgan, C. T. *Physiological Psychology* (New York: McGraw-Hill, 1943).

13. Sherrington, C. S. "On Binocular Flicker and the Correlation of Activity of 'Corresponding' Retinal Points" (*British Journal of Psychology*, 1, 1904–05, 26–60).

14. Sherrington, C. S. *The Integrative Action of the Nervous System*, 2nd edition (New Haven: Yale University Press, 1947).

THE PSYCHOLOGIST IN A CLINICAL SETTING

R. WEIL, M.D.
Department of Psychiatry, Dalhousie University

WHEN I RECEIVED your invitation for this meeting I wondered if it wouldn't be interesting to you to hear a psychiatrist's view of the function of a psychologist in a clinical setting. I certainly cannot promise that I will be able to give a satisfactory answer to the problem of clinical psychology as it exists today, but I hope that the discussion following my remarks will help me, and possibly you, to bring a greater clarity to this problem which is not only a matter of concern to us here in the Maritimes, but an area of great interest wherever psychologists and clinicians meet.

I have met psychologists and worked with them in different places and in different settings. As an administrator in a mental hospital I was interested in trying to fit the psychologist into the overall functions of mental hospitals. I had an opportunity to participate in the training programme with some of your colleagues. I met psychologists in a clinical atmosphere where psychotherapy and the teaching of psychotherapy were the main endeavours. I had an opportunity to experience the great value of the contribution of psychologists in an appraisal unit of the V.A. system in the U.S.A. I saw psychologists conduct group psychotherapy classes and I worked with them on problems of social research. All those experiences, of course, must necessarily colour my own opinion and therefore I wish to state at this point that I am speaking today only for myself and not for the Department of Psychiatry. In my contacts with you, I learned about the different schools and philosophies to which you are exposed. I met and worked with colleagues of yours of different calibres, of differing personalities. However, irrespective of the academic level attained by any one of them, I considered that my personal communication with them contributed more than did anything else to the problem of understanding the mutual task.

In the context of this paper I wish to define "Psychologist" as a professional person who applies psychological principles and techniques to the understanding, diagnosis, and treatment of those cases who come to the attention of clinicians.

When I speak about "clinical setting," I am referring to a place where sick people come for observation, diagnosis, and treatment. This definition is a wide one and is meant to be such, as I have in mind not only mental hospitals and psychiatric clinics, but also general hospitals and any places where people come with "psychosomatic problems." Your profession has now representatives in all of these places.

[1]Paper presented at the annual meeting of the Maritime Psychological Association, September 1951.

CAN. JOUR. PSYCH., **6**, 3 (1952).

In a way, you have made yourself indispensable. I think very few psychiatrists would practise today without the benefit of your contribution to the better understanding of patients and their problems in general. Your contributions to medical research are opening new avenues in this field; and medical men in general—not only psychiatrists—are learning to accept you because they have begun to appreciate your specific approach to the understanding of human problems. Even during the relatively short period I myself have been in psychiatry I have seen your numbers increasing very rapidly.

But the questions come to my mind, "Do we get the maximum benefit from your knowledge? Does the present relationship between you and the medical profession permit your optimum contribution to the sick person in our care?" I know we like to have you around, but I am afraid we don't know exactly how to use your knowledge most effectively. I realize that many of our colleagues use you and your knowledge as they would a laboratory technician and his methods; however, I feel that there is a great difference *in the kind* of contribution you make. One of the main differences rests in the fact that in evaluation of human beings from a psychological point of view there is no definable norm. The field of psychology can determine only broad bands in a wide spectrum called, arbitrarily, health or disease. Every human being is a unique individual and as such has to be evaluated with all his variables. Therefore, your methods of investigation, whatever tool you use, must be flexible and adjusted to the needs of the patients, as well as to the need of those who will utilize your professional judgment. When I speak about your contribution to the diagnosis and treatment of the patient, I don't mean that you and your test results should be just a support for us—one additional source which we can use to corroborate our diagnosis. I know we are often all too guilty of not considering you to be collaborators with us in dealing with the patient's problems, nor do we as psychiatrists and medical men accept realistically the possibility that you could, with us, be responsible for the welfare of our patients. Very often we act as though you were astrologers; we send our patients to you for a horoscope, then use this horoscope in our diagnostic and therapeutic considerations or discard it with some attitude of superiority. This attitude, of course, is anachronistic and dates back to the time when the psychologist was just a tester of human intelligence.

These last few points lead me to a short discussion of the common problems of the clinical psychologist and the psychiatrist. I believe the greatest factor in the efficiency of scientific endeavours of today is teamwork; and in order to make a team function optimally, a good understanding between the team members *as persons* is imperative. In most

places where I have seen clinical psychologists and psychiatrists working in the same setting, they were like people who came from different countries, very often speaking different languages and hardly ever meeting personally to share verbally their mutual problems. The differences in status and training, and the differences in the responsibilities assigned to them make the medical men assume the superior and all-important role. The lack of *common* basic psychological concepts frequently does not make this "marriage" any happier.

So far I have tried to describe the situation as I see it—the problem of the relationship of clinical psychologists and psychiatrists. I don't intend to leave matters there, but shall at least try to share with you some of my thoughts on this subject.

I consider the function of a psychologist in a clinical setting to be determined by three criteria: (1) the psychologist, (2) the psychiatrist, (3) the patient.

(*1*) *The Psychologist*

Under this heading I should mention first the *psychologist's personality.* Too often the clinical psychologist finds himself alone and isolated in an environment in which he must share responsibility with medical men, social workers, and other professional personnel. His security is continually threatened by strange concepts and unfamiliar medical and psychiatric techniques and terms. His ability to get along with people is important. His willingness to take responsibility and to share common functions with others, to be flexible and to learn the language and the concepts of another profession is essential. Under certain circumstances, he must be secure enough within himself to bear the stresses and strains imposed upon him by other professional workers who do not understand his functions and his contributions. He must be aggressive enough to make himself noticed but not too aggressive in imposing his own concepts and techniques upon others.

I could go on with the description of an ideal psychologist in a clinical setting but time doesn't permit me to mention all the qualities of the psychologist's personality that I consider to be desirable. *The psychologist's background and his training* will sooner or later influence his relationship with other professions: the philosophy underlying his teaching and his interpretation of the theory of testing will soon enter the question of his effectiveness in a *particular* setting. For instance, does he prefer to give psychological tests without any foreknowledge of the patient, or does he prefer to discuss the patient beforehand with the responsible therapist?

The psychologist's experience, not in testing only but in his past opportunities to work with people, will colour his relationship with

others. Also, in using the term I am referring to his experiences as a human being in daily life as well as to his professional academic career. His special interests are important. It is assumed, of course, that the psychologist who chooses to work in a clinical setting is sufficiently motivated and interested in this particular line of his profession. But even within this clinical work, the psychologist can choose many different endeavours. He may be interested in psychosomatic problems, or in problems of psychotherapy. He may be research-minded—and I think every psychologist should be—or he may be interested in specific therapeutic problems, such as the treatment of reading difficulties, the treatment of stammering, and so on.

Clarity of expression, that is, the ability to describe human beings on the basis of the test findings in understandable terms, is an additional positive factor in the psychologist's co-operative effort. The psychologist should have the facility to describe in understandable language the test behaviour of the patient, which very often is more important than the test results themselves. The psychologist's knowledge of psychiatric technique, diagnostic as well as therapeutic, is essential for his functioning in a clinical setting; and a good understanding of the mental mechanisms and the dynamics of the different pathological entities is an absolute requirement. Molly Harrower (1, pp. 12-13) discusses the prerequisites of a clinical psychologist in the following selected statements taken from several paragraphs:

> A psychologist may be considered clinical:
>
> 1. When he is able to take a responsible and unemotional stand on the all-important question of therapy.
> 2. When he ceases to consider himself as the infallible psycho-diagnostician.
> 3. When he knows that the rules for administering a test are not ends in themselves.
> 4. When he is willing to see himself as primary source material.
> 5. When he has sufficient perspective to select the relevant type of investigation for the problem presented by the particular patient.
> 6. When he is able to communicate his findings in an intelligible fashion, without recourse to high sounding technical clichés from text books, of the meaning of which he is actually uncertain himself.

(2) *The Psychiatrist*

For the functioning of a good clinical team, the qualities of the psychiatrist—his personality, his experience and background, and his interest—could be considered in the same terms as those employed in the paragraphs above dealing with the psychologist and his qualifications. In addition to these qualifications, the psychiatrist should have a working knowledge of the methodology and techniques of psychological testing. He should know what he can expect from the different tests. That doesn't mean, however, that he has to be able to administer all tests and be fully informed about the philosophy and theories behind them. The

psychiatrist, of course, will find co-operation with the psychologist profitable in the measure in which he accepts the psychologist himself on equal terms and the psychologist's findings as opinions important for the successful handling of their mutual task.

(3) *The Patient*

No clinic or hospital—be it general or psychiatric—can function effectively unless all of its activities are patient-centred. The main criterion for the effectiveness of any of these institutions is awareness on the part of the staff of the patient's needs. Let us now consider different kinds of patients who at one time or another may come in contact with the psychologist. From my experience, I feel that the patients who can benefit from contact with a psychologist can be divided into three groups.

(A) Patients who come for psychiatric and psychological evaluation, and whose evaluation is limited by a certain time period; for instance, criminals who are sent in to the clinic by the legal authorities, delinquent children and juveniles, defective children or those children with possible organic conditions of the central nervous system. Organicity may also be a question in some of the patients referred to us from neurosurgery, neurology, or other medical wards. Patients from other medical departments or from outside physicians may require psychiatric and psychological consultation. A main consideration in dealing with these patients is the time limit imposed upon us in our appraisal of their personalities. In order to reach a conclusion about these patients' diagnostic categories, their mental mechanisms, the prognosis and possible suggestions for therapy, in the limited time at our disposal, we have to use every suitable clinical method available. The contribution of the psychologist will be dependent on his own choice of a test battery. Since the psychiatrist very often knows nothing or very little about the patient, he is unable to ask a specific question of the psychologist and the psychologist himself will have to evaluate the problem the patient presents, at least superficially, and go ahead and administer the "appropriate" tests.

I personally saw the importance of this psychological contribution with this sort of patient when I worked in an appraisal unit at a V.A. Hospital in the United States. There, the patient was admitted and immediately informed about the different procedures he would have to go through before he would be properly diagnosed and the appropriate treatment determined. Such a patient might have been seen first by the social worker, the medical man, or the psychiatrist and referred immediately to the psychologist, or he might have been appraised solely by the psychiatrist after a physical examination. The whole procedure at this

appraisal unit was very flexible: at times the psychologist would interview the patient for a short period in order to determine what his main problem appeared to be. From my experience at the present time at the Victoria General Hospital, I can state that at times we are faced with the necessity for quick evaluation of patients whom we may see for only a short period, and there the help of the psychologist is invaluable.

(B) The second group of patients is not as easy to define as the first. With them, we are not so much pressed by time as by the problem of diagnosis. In many patients we are presented with the problem of a differential diagnosis. There are cases that come to the out-patient department or that are referred from other medical wards for consultations and suggestions about treatment. In order to get at least a superficial view of the patient's problem, we first take a history. At times we resort to social service investigations to get a better picture of the patient's difficulties, but there are occasions on which psychiatric and social service interviews are unable to clarify the character and nature of the patient's psychological problem. In these cases, the psychiatrist has more specific questions to ask the psychologist than in the case of patients belonging to category (A). The psychiatrist has had longer contact with the patient; he has had opportunity to evaluate him better than patients of group (A) but he has not been able to come to any definite diagnostic conclusions. In spite of this fact, he will be forced to take some administrative or therapeutic steps. Questions the psychiatrist will ask in these cases might, for instance, be, "Can psychological tests determine whether this patient is depressed or whether he suffers from a schizophrenic process?" or "Could you, as a psychologist, help us to differentiate in this case between the diagnosis 'obsessive compulsive neurosis' and 'schizophrenia'?" or "Is the difficulty of this patient due primarily to a psychological disorder or to an organic condition?" While problems presented by the (A) group require a somewhat routine procedure, in this second group the testing becomes a more focused endeavour. This fact has several implications. First of all, the psychologist has to understand the questions of the psychiatrist. He also has to know what kind of tests are likely to give an optimal answer to these particular focused questions. On the other hand, he has available a great deal of data which has already given him some information about the patient. This information, of course, the psychologist will utilize only if he belongs to the school which does not avoid "contamination" by other data than those obtained by tests. Dealing with patients of the second category, therefore, requires a greater degree of communication between the psychologist and psychiatrist than dealing with patients in category (A).

(C) No clinic or hospital today is organized merely to diagnose and

appraise patients from a psychological, psychiatric point of view. Everywhere therapy is applied and all forms of psychiatric treatment administered, be it by some physical method, such as shock treatment, prefrontal lobotomy, and so on, or by some form of psychotherapy. The administration of the different forms of therapy will, of course, depend on the diagnosis of the particular case. However, I don't want to discuss those who require physical forms of treatment. Of course, we administer psychological tests to every case, but we don't require the psychologist's contribution in the case of most patients who are to receive electric shock therapy, subcoma, or coma insulin treatment. In this category (C), I think belong primarily all those patients who require and/or are receiving psychotherapy. Some of these cases have already undergone testing in the process of the diagnostic appraisal, but during therapy the psychiatrist very often is faced with very important questions in the solution of which the psychologist can be of great help.

Psychotherapy is a process in which the therapist gives the patient the opportunity to relate himself to a person whom he can trust, in which the therapist represents reality to the patient, and in which he also helps the patient to bring out and understand his pattern of behaviour and thought, and so assists the patient to adjust more effectively and realistically to the intra- and interpersonal problems of daily living. As psychotherapy proceeds, problems will arise which will either slow down the therapeutic process or stop it entirely. To be effective and helpful, the therapist should be able at all times to evaluate both the process of therapy and the goal to which the patient is capable of travelling. For this purpose, he has to answer many questions, and in the clarification of these the psychologist can again be helpful. For instance, "What personality resources does this patient possess?" "What specific functions and performances of this patient have been influenced by the therapeutic process?" "Is the present anxiety the symptom of a decompensation of previous defence mechanism, or is it only a transitory phenomenon which could be largely ignored during therapy?" "Is the slow-down in the therapeutic process due to some homosexual tendencies which recently came out in the therapeutic relationship?" "Has the breakdown of more superficial defence mechanisms during the therapeutic process brought out some deep-lying schizophrenic mechanisms, and therefore should the psychotherapeutic approach be restricted only to supportive measures?" "What are the predominant defence mechanisms of the patient?" "Is he, for instance, using chiefly the mechanism of denial and repression, or is he dependent for the most part on obsessive compulsive attitudes?" "Are the developing depressive features in a patient his reaction to the uncovering of conflicts, or are his present low spirits

due to a decompensated character disorder?" "Is a confessed insight of the patient the result of a corrective emotional experience or is it the product of intellectualization?"

In therapy, with every patient many problems present themselves. The clarification of some of the problems would take many hours and so would slow the process itself. My question is: "Can the techniques of psychology help us to clarify the difficulties in therapy more rapidly?" I realize that you don't have methods available as yet which would answer all of these and other questions, but I feel that if they require clarification it is not so much owing to the lack of techniques and methods as it is to the difficulty in communication between our two professions. It is also possibly owing to the lack of psychotherapeutic experience of the psychologist.

Another problem enters this specific consideration, and that is the fact that psychological testing itself is a specific experience for the patient and as such is of therapeutic significance; we are therefore at times hesitant to send a patient undergoing psychotherapy to a psychologist, because we are afraid that the original rapport may be disturbed by test procedure. The testing of patients in this category definitely cannot be done blindly. The psychologist has to know in what stage of therapy the patient happens to be at the moment. He has to understand exactly what it is that the psychiatrist wants to know; he therefore needs a great deal of information about the patient so that he can choose most readily those tests which will give him the specific answer sought by the therapist. For instance, I have in mind the administration of the T.A.T., in which some of the cards might have a traumatic effect on the precariously balanced emotional homeostasis of the patient. In dealing with these therapeutic problems, the psychologist has ceased to be a tester and has become a co-operator in therapy, and therefore he has to understand something about the principles of therapy, and possibly should himself be able also to do some psychotherapy under psychiatric supervision and guidance.

In dealing with patients in co-operation with the psychologist, I have become progressively conscious of the importance of easy communication between the psychiatrist and the psychologist. In category (A) in which a practically blind evaluation of the patient's problem is to be made, the psychologist can give his opinion on the basis of his test results and the patient's test behaviour, with practically no communication with the psychiatrist. In category (B) in which diagnostic problems are most important and in which the psychiatrist may ask more focused questions, the psychologist must not only understand the question of the psychiatrist, but must also be able to give a diagnostic appraisal of the patient

in terms and concepts which are meaningful to the psychiatrist. In category (C), that is in the psychological evaluations of the therapeutic processes, the techniques and the tools used are not as important as the clinical judgment of the psychologist and the communication between the psychologist and the therapist. The significance of the psychologist's contribution to therapy may lie mainly in the fact that the patient relates himself to the administrator of the tests quite differently than he does to the therapist. With the psychologist the patient becomes test-centred, and consequently the human relationship is pushed more into the periphery. In the therapeutic process the patient is more aware of his relationship to another human being.

My thoughts on the function of a psychologist in a clinical setting would not be complete if I did not say a few words about research. In the area of research, many problems are still awaiting answers, and some of these answers cannot be reached by any profession alone. In fact, I feel that most research in any of the human sciences will have to be done in the context of teamwork, and here in a clinical setting is an ideal situation in which psychologists, psychiatrists, social workers, and other professional personnel can develop ever more insight into the functioning of the normal and the sick personality. Such co-operative effort in itself provides a new experience for all team members and this experience in turn can create an atmosphere in which clinical efficiency can and will improve greatly, to the benefit of those who are under our care.

In summing up, I wish to convey to you again the following thoughts. The field of clinical psychology is rapidly widening and infiltrating all areas of clinical activities. The functions and activities of a clinical psychologist are as yet poorly outlined. The knowledge, the techniques, and methodology of the clinical psychologist are not at present fully utilized for the benefit of our patients. Adequate communication between clinical psychologists and psychiatrists is not sufficiently established to produce optimal co-operative results. The training of the psychologist, especially in psychotherapy, is insufficient, and so is the training of the psychiatrist in psychological testing and psychological research. Adequate communication and co-operation is demanded by the needs of our patients, who I feel fall principally into three main categories:—(1) Those who have to be quickly evaluated; (2) Those who present diagnostic problems; (3) Those who in psychotherapy present problems in treatment. Last but not least, I have stressed the importance of common research.

In conclusion, let me quote a statement of my former teacher Dr. W. C. Menninger, speaking about clinical psychology: "I suspect the situation

will have to evolve to a point where the psychologist can accept the psychiatrist as the quarterback of a team that works together, in which neither individual needs to be concerned with threats from the other. We must reach a stage in our evolution when the bugaboos of status, jurisdiction, equality and subordination become dead issues" (2, p. 18).

REFERENCES

1. HARROWER, MOLLY R. "The Evolution of a Clinical Psychologist," *Training in Clinical Psychology* (New York: The Josiah J. Macy Foundation, 1947).

2. MENNINGER, W. C. "The Relationship of Clinical Psychology and Psychiatry" (*Bulletin of the Menninger Clinic*, 14, 1950).

IRRATIONAL FEARS IN THE DOG

RONALD MELZACK[1]
McGill University

SPONTANEOUS FEARS of strange objects, which are not associated with a more primitive cause of fear, have been adequately described in the chimpanzee. The information provided by Köhler (10), Yerkes and Yerkes (13), and Haslerud (4), has been recently extended by Hebb and Riesen (8), and Hebb (6). However, no comparable experimental knowledge exists for the dog, although such fears in the dog have been the subject of speculation since the time of Darwin (2) and Romanes (12).

The purpose of this paper is to make a preliminary study of fear in the dog. It is hoped that these data may help in the evaluation of current theories of the nature of fear, and toward the construction of a comparative psychology of emotion.

EXPERIMENTAL METHOD

Criteria of fear behaviour. The criteria of fear behaviour which were employed in evaluating the experimental observations were: (1) when an approach of the object towards the dog produced an excited movement of its head, so that the dog's head was pointing away from the object, at right-angles to it; (2) when an approach of the test object caused the animal to crouch down and to lower its head and press its ears against its neck; (3) when the animal ran away as the object approached, always keeping it outside of its field of vision; (4) when the animal ran to the farthest part of the room, and maintained the greatest possible distance from the object when it was brought closer.

A group of psychologists who were working with dogs considered the above criteria, and believed them to be valid indications of fear in dogs, (1) being minimal and (4) maximal, as indications of the degree of fear. A weighting system was devised, with a maximum score of 4 points per presentation of a stimulus object, which is comparable to the criteria used by Haslerud (4) with chimpanzees. The weighted scores can be stated in terms of a percentage of the total possible score, thus permitting a rough comparison of the various objects in ability to induce avoidance behaviour.

Test objects. Because this was a preliminary study, no attempt was made to cover the entire range of objects and situations which produce spontaneous fear in dogs. The test objects fall into two categories: (1) stationary objects, and (2) objects with movable parts. They are listed

[1]The author wishes to acknowledge gratefully the advice and guidance of Dr. D. O. Hebb. This research was assisted by a grant from the Dominion-Provincial Mental Hygiene Program.

in Table I, and the percentage of avoidance from the total possible score manifest by the dogs is indicated for each object.

Subjects. Twenty-six home-reared dogs were used, including Dalmatians, Scottish terriers, collies, and cocker spaniels. They ranged in age from 2 months to 12 years. The range of experience of these dogs was considerably wider than that of laboratory dogs. However, since the complete histories of the dogs were known, it was reasonably certain that the test objects were not associated with previous painful experiences.

Procedure. To avoid the complication of a strange environment (which itself might cause fear), testing was carried out in the homes of the animals. The dog was led into a room illuminated by an ordinary lamp, near which lay the test object. Each object was presented for a total of three minutes. After a 15-second period in the room, the object was moved slowly toward the dog for another 15 seconds. The objects that had movable parts were set into motion slowly over a period of five seconds. After being then moved toward the dogs for another 15 seconds, they were placed on the floor for the remainder of the test period.

In the case of the life-size bronze deer, the experimenter made use of the large statue of a deer situated on a street corner. The dog ran after the experimenter, and upon turning the corner, ran close to the deer.

No more than three objects were presented in a single day, and they were presented in random order varying from dog to dog. No more than eight objects were presented to any dog.

Observations and Results

In a total of 98 presentations with stationary objects, 43 avoidance responses and 16 aggression responses were recorded. Thirty-nine presentations elicited either a diffuse excitation (containing rudimentary forms of avoidance and aggression), which failed to meet the criteria for avoidance, or no emotional response. The total avoidance score was 86, with a mean of 0.9 per presentation.

In a total of 137 presentations with test objects with movable parts, 112 avoidance responses and eight aggression responses were recorded. The total score for these objects was 369, with a mean avoidance score of 2.7 per presentation.

The difference between the avoidance scores for the stationary objects and scores for the test objects involving movement was found to be significant at the 0.1 per cent level.

The different degrees of avoidance response to the skulls, and to the opening of three sizes of umbrellas, indicate that a factor of size is contributing to the dog's avoidance of innocuous objects. This was also

TABLE I

THE PERCENTAGE OF AVOIDANCE RESPONSES (FROM THE TOTAL POSSIBLE POINTS) FOR THE TEST OBJECTS

Stationary objects	Percentage of avoidance	Objects with moving parts	Percentage of avoidance
Mounted cobra	12.5	Chair moved by threads	37.5
12" toy lizard	18.8	Soap bubbles that burst	40.6
Skulls:		Car moved by wire in hand	65.6
Human	25.0	Balloon, 12" in diameter	71.9
Chimpanzee	12.5	Moving man draped in sheet	59.4
Dog	0.0	Mechanical turtle	62.5
8" toy horse	21.9	Electric train	50.0
24" toy collie	25.0	Umbrellas, opened to:	
Mask on familiar person	50.0	36" spread	80.8
Life-like bronze bust of a man	15.0	30" spread	77.9
Horse skin	50.0	24" spread	65.4
Life-size bronze deer	50.0		

shown in the test in which a balloon was blown up slowly in front of the dogs. A generalized excitement in the dogs was observed as the balloon increased in size. However, when the balloon became large (six to eight inches in diameter), the dogs invariably jumped back a few feet. Most of the dogs continued to run away from the balloon when it was moved closer to them.

The manner in which the object is presented to the dog is important in producing the fear response. When a horse skin, which still retained the shape of a horse, was presented to the dogs in its usual capacity as a rug, no reaction was obtained. However, when the skin was placed over a sofa, giving the appearance of an upright horse, the fear response was produced. When later replaced on the floor, no reaction was observed.

The aggression which appeared in these tests was characterized by an excited approach toward the object, although in no test was any damage done to the stimulus object. This type of quasi-aggression ended as soon as the test object was hidden from the field of vision of the dog. The aggressive manner was especially apparent with the smaller, stationary objects such as the dog skull, the stuffed cobra, and the toy horse.

SUPPLEMENTARY OBSERVATIONS

In order to be sure that the factor of motion in the procedure was not by itself a stimulus sufficient to elicit the avoidance response, rubber balls and pieces of wood (corresponding in size to the skulls, toy horse, and so on) were moved slowly toward the dogs. The procedure was the same as that employed in the main experiment, but no avoidance was observed. From this comparison it appears that the dog makes immediate

discriminations of visual patterns in three dimensions, despite the fact that rather prolonged training may be necessary before pattern discrimination can be demonstrated by the usual methods (1, 9).

Some at least of the test objects could not have been discriminated by smell, as with the bronze deer, or the mechanical turtle, and in the course of the work it was frequently clear that visual cues were affective before olfactory ones could have been. Vision also can dominate olfaction: fear of a modelled human head was observed although the model was completely wet with dog urine.[2]

To better understand the contribution of motion, the tests with the toy car, electric train, and umbrella were repeated. A sudden movement of these objects produced greater avoidance than a slow, steady increment in speed.

With a group of dogs which had not seen the umbrellas before, it was found that no fear occurred when an opened umbrella was closed before them for the first time. However, after presenting an open umbrella to the dogs for 15 minutes per day for 9 days, avoidance responses were produced in three of five subjects when the open umbrella was closed on the tenth day.

Individual differences were evident in all of these tests. This was further exemplified when a cardboard box was placed over a moving toy mechanical car, giving the appearance of the box moving along the ground. The result was an excited alternation of avoidance and aggression in three of the dogs, which continued for about four three-minute test periods, until either avoidance or aggression developed as an immediate consistent pattern of response. Two of the animals attacked the car and box, upsetting it; one dog continued to avoid it.

The Nature of the Irrational Fear

The results and observations of the present investigation elucidate a number of points concerning the nature of the irrational fear.

That a type of learning, other than conditioning, is prerequisite for the "spontaneous" avoidance response is indicated in the test in which an open umbrella had to be presented for a period of time before avoidance was produced by closing the umbrella. Since no fear response occurred after closing the umbrella on its first presentation, learning of the type suggested by Hebb (7) appears to have taken place during the periods of visual stimulation by the test object. Similarly, the test with the car and cardboard box indicated that the integrated part of an emotional

[2]Personal communication from Mr. W. Heron, in this Laboratory.

pattern of response (that is, attack or retreat) was established at least in part by learning.[3]

Two variables of the stimulus situation that contribute to spontaneous fears in the dog are: (1) the size of the object, and (2) the speed with which an object is set into motion.

(1) *Spatial factors.* The importance of size is demonstrated by the decrease of avoidance responses with a decrease in the size of umbrellas and skulls presented. Furthermore, the largest percentages of avoidance were produced by the large objects such as the bronze deer and the horse skin, while the least avoidance (although the most aggression) was produced by the dog skull, the cobra, and other small objects.

(2) *Temporal factors.* The degree of avoidance in the dogs was found to be affected by the suddenness of movement, indicating the important liminal and subliminal fear values of motion.

Spatial and temporal factors, however, in order to provoke avoidance or aggression, must be supported by the factor of "awfulness," described by Köhler (10). This fear of the unfamiliar and the strange is apparent in the situations involving the horse skin, a chair moving by means of a thin thread, and a mask on a familiar person.

The Comparative Psychology of Fear

In a recent paper, McBride and Hebb (11) have suggested that motivation and emotion may provide a better index of phylogenetic relationships among animals than problem-solving and intelligence tests. McBride and Hebb compared the porpoise with other mammals and concluded that the behavioural hierarchy was: man, chimpanzee, porpoise, cat or dog, and laboratory rat. The present investigation supports the validity of placing the dog below the chimpanzee and porpoise, if we use as criteria the susceptibility to fear of innocuous objects, the variability of behaviour (among individuals, and of a particular individual from time to time), and the length of period of adaptation to exciting innocuous objects.

However, in comparing two closely related species such as the dog and horse, Grzimek's (3) description of the strength of fear induced in the horse by stationary test objects would place the horse above the dog in susceptibility to fear of innocuous objects. In order to obtain a complete picture of the behavioural level of an animal such as the dog, the other senses, especially the olfactory sense, must be carefully investigated.

Although the dog's visual acuity is poor when compared to the other

[3]In material to be published at a later date, the effects of learning will be shown to be even more important than here indicated.

mammals (2, 12), it has been observed that motion in the presentation of an object may make that object more discriminable from the rest of the field. McBride and Hebb (11) may not have considered this point, and thus they have stated that spontaneous fears of innocuous objects seldom occur in the dog. The present investigation, however, indicates that dogs manifest spontaneous fear of a wide variety of innocuous objects; thus the dog would place well above the rat in complexity of emotional behaviour.

Summary and Conclusions

By testing home-reared dogs in surroundings familiar to them, it was found that spontaneous fears can be visually aroused in the dog by a large number of innocuous objects. The number of avoidance responses made to test objects with moving parts was significantly greater than the responses to stationary test objects. Learning played an important role both in the development of spontaneous fears, and in the genesis of a particular type of response in a given stimulus situation.

Temporal and spatial variables contribute to the fear-provoking character of the test situation, but they must be supported by the factor of strangeness. A comparison of the spontaneous fears of the dog with those of other mammals indicates that the dog places above the rat, but below the porpoise in complexity of emotional behaviour. It is dangerous to assume a hierarchy of psychological complexity without a knowledge of the degree of development of each of the senses, or without the consideration of phenomena which have attention-getting value to a species.

REFERENCES

1. Buytendijk, F. J. J. *The Mind of the Dog* (Boston: Houghton Mifflin Company, 1936).

2. Darwin, C. *The Descent of Man* (New York: The Modern Library, 1936).

3. Grzimek, B. "Scheuversuche mit Pferden" (*Zeitschrift für Tierpsychologie*, 6, 1944, 26–40. *Psychological Abstracts*, 22, 2036).

4. Haslerud, G. M. "The Effect of Movement of Stimulus Objects Upon Avoidance Reactions in Chimpanzees" (*Journal of Comparative Psychology*, 25, 1938, 507–28).

5. Hebb, D. O. "Emotion in Man and Animal: an Analysis of the Intuitive Processes of Recognition" (*Psychological Review*, 53, 1946, 88–106).

6. Hebb, D. O. "On the Nature of Fear" (*Psychological Review*, 53, 1946, 259–76).

7. Hebb, D. O. *The Organization of Behaviour: a Neuro-psychological Theory* (New York: John Wiley and Sons, 1949).

8. Hebb, D. O., and Riesen, A. H. "The Genesis of Irrational Fears" (*Bulletin of the Canadian Psychological Association*, 3, 1943, 49–50).

9. Johnson, H. M. "Visual Pattern-discrimination in the Vertebrates–V. A Demonstration of the Dog's Deficiency in Detail-vision" (*Journal of Animal Behavior*, 6, 1916, 205–21).

10. KÖHLER, W. *The Mentality of Apes* (London: Kegan Paul, Trench, Trubner, 1925).

11. MCBRIDE, A. F., and HEBB, D. O. "Behaviour of the Captive Bottlenose Dolphin, *Tursiops truncatus*" (*Journal of Comparative Physiological Psychology*, 28, 1948, 111–23).

12. ROMANES, J. G. *Mental Evolution in Animals* (New York: D. Appleton and Company, 1884).

13. YERKES, R. M., and YERKES, A. W. "Nature and Conditions of Avoidance (Fear) Response in Chimpanzee" (*Journal of Comparative Psychology*, 21, 1936, 53–66).

Arms of the Law. By MARGERY FRY. Toronto: Longmans, Green & Company, 1951. Pp. 255. $2.00.

THIS BOOK encompasses two principal themes. First, there is an historical account of penological methods, and secondly, there is a plea for the reader to join the Howard League for Penal Reform. The author has not permitted her role of reformer to influence her role of historian.

The effort to interest the reader in the work of the Howard League is contained in a separate section towards the end of the book. The work of the Howard League through its affiliated societies in Canada in the field of penal reform and rehabilitation of prisoners is widely known and there will be general agreement with the author's outline of the important work which is being conducted by this organization.

Most of the book is devoted to a description of the historical development of penological concepts, the results of investigations into the causes of crime, and the transition from punishment dominated by revenge to a system of penology based upon the protection of society with the treatment and rehabilitation of the offender.

Discussing the criminal statistics in the United Kingdom, the author points out (page 101) that the volume of crime remains far above that of 1938 and stresses the obligation to use scientific lines of research and experiment rather than intensify the use of methods which are proving inadequate.

A consideration of the volume as a whole would offset the view that the efforts of those who aim at penal reform are motivated primarily by a "soft" or humanitarian attitude. These efforts are motivated rather by enlightened self-interest on the part of the community in the belief that these efforts are more successful in the control of crime than an antiquated system dominated by revenge.

This theme is expressed in various ways by the author. For example, on page 199:

> The treatment of offenders has for some years been increasingly influenced by people whose attitude is, as a stipendiary magistrate has recently put it, "experimental and medical." But the officials and others who are prepared to take such an attitude are constantly cramped by (i) the drag of public opinion and the trammels of the law, (ii) the antiquated buildings in which much of their work must be done, (iii) staffs which are only partially aware of, or in sympathy with, any but the deterrent method of checking crime.

The appendices A and B contain information relating to the electroencephalogram. Since this book is written primarily for the informed general reader rather than for the scientific specialist, there may be doubt about the value of including these appendices. The interpretation of

electroencephalograms is difficult at best and their interpretation in relation to antisocial conduct is a relatively uncharted area of investigation.

This work appears to be a reliable and authoritative presentation of the historical development and changing concepts in the field of penology.

K. G. GRAY

University of Toronto

The Dynamics of Psychological Testing. By MILTON S. GURVITZ. Toronto: The Ryerson Press, 1951. Pp. xii, 368. $8.00.

THIS BOOK attempts to answer, by the case method, the familiar questions of "What can the clinical psychologist do as a psychodiagnostician?" and "How exactly is this done?"

The first part of the book is devoted to a discussion of clinical test instruments, personality theory, and diagnostic formulations. Having armed himself with the now-classical test rationale, a Freudian viewpoint, but depriving himself of case-history materials, the author sets forth in detail his test results, analysis, and his report to the referring therapist. The seventeen cases reported were those referred to him for psychological evaluation during a one-month period at the Hillside Hospital. These cases presented challenging problems from a diagnostic and prognostic point of view since referrals to the author were characteristically made on this basis.

An interesting feature is that the psychological reports are set up in the form of discrete assertions with an indication of the test result basis, and are followed by the comments of the therapist as regards agreement or disagreement. A brief case history follows the report and a final evaluation of the relation of assertions of the psychologist and therapists is given for each case. The extent of agreement between therapist and psychologist is obviously very great. In conclusion the author makes a not too serious attempt to demonstrate statistically the value of psychodiagnostic testing.

The book has considerable instructional value for student and practising clinical psychologist. Personal participation of the reader is particularly invited by the full presentation of test data. The breakdown of report material certainly aids the reader in following the author's inferences and in checking these against clinical impressions. One danger in this method of presentation is of reducing to a common level the significance that the various sorts of assertions have for the understanding and treatment of the patient under consideration. A tendency to reduce the whole matter of psychological evaluation to a game emphasizing the

number of correct answers may well be encouraged. To some extent, this is encouraged by the reports made by the author which concern themselves in detail with the nature of intellectual functioning and psychodynamics but which include only one point as recommendation.

As a presentation of the method and status of clinical testing to-day this book does an excellent job. The achievements and shortcomings of the book are not peculiar to it but characterize the field.

BRUCE QUARRINGTON

University of Toronto

Mental Tests in Clinics for Children. By GRACE H. KENT. Toronto: D. Van Nostrand Company, Limited, 1950. Pp. xii, 160. $3.00.

THE PURPOSE of this book is to present the author's unique viewpoint on mental testing to the advanced student in clinical psychology. She believes that the emphasis on norms has been parallel to the tendency to lose sight of the individual. In spite of her light dismissal of statistics one cannot disagree with her valuable recommendations gained from her own clinical experience.

The author begins by presenting the various reasons why children are referred to the clinic and emphasizes the part psychological tests play beyond finding the child's mental level. The details of the clinical examination are then summed up, and the author points out the need to adapt the examination to the child.

Her discussion thus far has been building up to her main point of view presented in Chapter III, "Sources of Error in Use of Tests," and Chapter IV, "The Composite Scale Versus the Battery." In brief, she suggests that the quantitative aspects of mental testing have received undue emphasis. Errors in testing result from administering routine tests and taking scores too literally, allowing little recognition of individual differences.

In the remaining chapters she discusses the various types of tests, the psychological report, and what she considers the criteria for construction of clinical tests.

Clinical psychologists in schools, guidance centres, and service clinics will find the book thought-provoking and useful. Chapters III and IV should be of interest to any clinical psychologist.

DORIS C. WARNER

Queen's University